The Impact of Transitory Income on Birth Weights: Evidence from a Blackout in Zanzibar

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Abstract

Do transitory income shocks affect infant health? I find evidence that birth weights fell following a temporary income reduction caused by an unexpected, month-long blackout in Zanzibar. Relying on 350 household surveys collected during field work, I show that the 2008 blackout reduced labor supply of workers in electricity-dependent jobs by an average of 25%, with no effect on workers in other sectors. The income shock was temporary. Using over 20,000 birth records from a maternity ward, I document a reduction in the average birth weight of children exposed to the blackout while *in utero*, and an increase in the probability of low birth weight. Supporting a causal interpretation of these results, the reduction in weights is correlated with measures of maternal exposure to the blackout. In particular, reductions in birth weights were largest among children from wards with intermediate levels of employment in electrified sectors. The two causes that are most consistent with these results are a blackout-induced decline in maternal nutrition, and maternal stress. Alternative explanations are examined, including the possible effects of a temporary fertility shift. It is shown that the blackout increased births, but that selection into pregnancy cannot explain the drop in birth weights.

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1 Introduction

This paper uses an unusual natural experiment in East Africa to show that transitory household income shocks have a negative effect on the health of infants. In particular, it shows that a one-month drop in earnings during the first six weeks of gestation is associated with significantly lower infant health, as measured by birth weights. More importantly, it is also associated with a large increase in the proportion of children born with low birth weights. Since health at birth is correlated with future child health and adult outcomes such as educational attainment, chronic disease, and decreased life expectancy, transitory income during gestation has significant long-term effects.

It is not at all obvious why a temporary spell of lower income should affect birth weights in the first place. While the medical literature has demonstrated a connection between maternal stress and food intake on pregnancy outcomes (Kramer 1987), idiosyncratic earning shocks should not affect consumption when credit markets are perfect. In that context, any observed changes in birth weights would likely driven by substitution effects such as behavioral changes or maternal stress. Since healthy behaviors seem to be small and counter-cyclical (Ruhm, 2000, Dehejia and Lleras-Muney 2004), we should expect little or no effect of transitory income on birth weights. In the context of a developing country, however, credit market imperfections are such that temporary earnings shocks could reduce maternal food consumption and impact fetal growth. The inability of households to smooth over idiosyncratic shocks could, therefore, help explain part of the birth weight differential between developed and developing countries (Behrman and Rosenzweig, 2004).

The results of this paper are derived from data generated in the aftermath of a unique natural experiment in the island of Zanzibar, Tanzania. In May 2008, an unexpected rupture in the undersea cable that brings power to the island caused a blackout lasting four weeks; the outage affected anyone with a power connection. I use data from a household survey collected in November 2008 to show that the power cutoff caused a large decline in the income of households employed in occupations that require the use of electricity. In contrast, the power loss had no discernible impact on work and earnings of households engaged in traditional activities with no reliance on electricity. Overall, I find that the blackout caused an income shock to 30% of households, with 22% having estimated losses greater than 10% of their monthly income. While sharp, these declines were purely transitory: the survey evidence indicates that virtually all households returned to their pre-blackout levels of labor and income within five months of the event. Birth weight results come from administrative birth records originating from the largest maternity ward of the island, which report information on birth outcomes as well as some demographic maternal characteristics. The records show an average reduction of 75 grams (2.6 oz.) in birth weights for children that were likely exposed to the blackout in the first six weeks of gestation, as well as to those that were conceived within a month from the end of the blackout. I find fewer heavy children (weighing above 3.5 kg) and more children with Low Birth Weights (LBW— defined as weighing below 2.5 kg.) among this cohort.

To support a causal interpretation of my main findings, I link births to the ward of residence, and utilize the 2007 Zanzibar Labor Force survey to construct ward-level proxies of likely household exposure to the blackout. Lacking precise measures of workplace electricity, I use the share of workers employed in sectors that use electricity. I show that reductions in birth weight among children exposed early in the gestation period were linked to workplace electricity use, suggesting that the shock to parental earnings affected pregnancy outcomes. In particular, I find that the relationship between birth weights and exposure to the blackout is U-shaped. No reductions were observed in wards with little or no exposure. Birth weights fell more in areas with higher share of employment in electrified sectors, but were normal in areas with the highest share. This may be due to the fact that the latter areas are also the wealthiest and, consequently, the most likely to be able to smooth out the economic shock.

Reductions in household earnings could have had an impact on birth weights in the early gestation cohort through lower intake of calories or micro-nutrients, or more maternal stress during the critical first stages of gestation. The results are consistent with both, although it is somewhat unlikely that the entire effect is driven by reductions in calories. That is because the implied income elasticity of calories necessary to generate the birth weight reductions observed in the data and measured under strong assumptions to be between 0.38 and 0.51 would be too large.[1]

While the pattern of birth weight losses is consistent with an explanation that relies on income shocks, a direct causal effect requires ruling out the presence of confounders and selection bias. A first source of concern is the presence of other shocks preceding or following the blackout that could have had an effect on birth weights. There were no reported interruptions or disruptions to the health system, and no other shock hit the island. [2] On the other hand, world prices for tradable food commodities reached historic highs in 2008, and such prices could have had a direct effect on birth outcomes. The tests I carry show little or no impact of such prices on the affected cohort, mostly because prices remained high for several months before and after the blackout. Such persistence in food prices cannot be reconciled with the sudden drop in birth weights.

A second set of worries is that the blackout itself might have caused other changes in the economy and household behavior that are conducive to lower birth weights. For instance, it might have compromised food preparation at home, resulting in a different (and perhaps less nutritious) bundle of products consumed. This is unlikely. 96% of households use firewood and charcoal for cooking, with most of the remaining household using paraffin or kerosene (SMZ, 2005). Moreover, perishable foods were available during the blackout, since markets for unprocessed perishable foods (including fish, meat, and milk) do not rely on electricity. As an additional check, I show that price movements of locally produced agricultural goods are not consistent with

[1] While this is consistent with the 0.3-0.5 elasticity of calories with respect to permanent income range estimated elsewhere (Subramanian and Deaton 1996), it is likely to be too high for transitory income. For instance, Thomas and Stillman (2007) measure the elasticity of consumption with respect to temporary shocks at a smaller 0.1 among Russian households. This is likely to be a lower bound for Zanzibari households, which are much poorer and more credit constrained. Another possible mechanism is excessive maternal calorie expenditure, which is unlikely in light of the lack of additional domestic or market work during the blackout reported by households surveyed after the event.

[2] While exposure to Ramadhan has been shown to affect birth weights in other locations with a significant Muslim concentration (Almond and Mazumder, 2008), the timing of the blackout was such that the affected birth cohort had no in-utero exposure to Ramadhan.

supply-side shortages. Moreover, to ensure that the birthweight effects of the blackout are not transmitted through some other channel, I show that ward level measures of domestic electricity use are uncorrelated with the reduction in birth weights.

A final concern is that the blackout caused selection into hospital admissions. There are two noteworthy potential sources of selection. First, the blackout might have caused a breakdown in the health care system, leading to a temporary change in the mix of hospital clients. That was not the case. Reports by the ministry of health indicated that "the immediate effects of the power failure on the provision of health services and prevalence of diseases have fortunately been fairly limited" (Straheler-Pol and Haji, 2008)[3], with no reported problems with maternity or antenatal care. A more serious source of selection is the effects of the blackout on fertility, caused by a temporary decline in the opportunity cost of having children. The evidence points to a cohort of children born eight to ten months after the blackout that was 11% larger in number than expected. If the blackout increased fertility, then lower birth weights could be the result of unwanted pregnancies or of selection into fertility for women at a higher risk of unhealthy babies. I argue that this possibility is unlikely by showing that lower birth weights are recorded across all maternal characteristics, and not only on those associated with selection.[4]

This paper contributes to our understanding of the causal relation between income and health in general, and children's health in particular. The relationship with permanent income is well studied: health improves with higher income [e.g., Lindahl (2005) using data from lottery winners], and birth weights decline after the loss of a parentÕs job (Lindo, 2010); see also Strauss and Thomas (2007) for a comprehensive overview. Papers that look at the effect of the business cycle on birth outcomes are closer to this paper. Dehejia and Lleras-Muney (2004) link individual births in the US with an aggregate variable, the unemployment rate. They find that birth weights are countercyclical, with improvements in birth weights generally arising from selection into fertility and better health behavior. Conversely, van den Berg et al. (2006) find higher mortality rates for those born during a recession in the Netherlands in the 20th century, suggesting that recessions are bad for lifetime health because they are bad for newborn health. While these papers provide some evidence of the impact of aggregate transitory shocks on neonatal health, they do not directly show whether recession affect health through transitory income, permanent income, or some other externality from recessions.

Due to the quasi-experimental nature of the context, this paper also provides some unusual insights

[3] The report mentions that hospital generators functioned properly throughout the period, but that there was a small temporary decline in vaccination rates due to problems in the supply of vaccines to remote outposts. The main concern of health authorities—an outbreak of cholera—did not materialize.

[4] Conditional on the blackout having a negative effect on birth weights, there might be some adverse selection from problem pregnancies that would have been delivered elsewhere had the blackout never occurred. This possibility arises from the fact that the hospital in question is a referral hospital. If that were the case, the birth weight effect measured here could be overestimated.

to the literature of fertility and infrastructural development. In the first case, it leads some credence to the belief that electricity can reduce fertility rates by increasing the opportunity cost of procreation.[5] In the second case, the paper documents the effects of blackouts on household behavior. It complements other papers on blackouts that focus on the effects on firm production and profitability (Adenikinju 2003).[6]

The remainder of the paper is structured as follows. Section 2 provides some additional background information on blackouts, birth determinants, and the nature of the Zanzibar event. Section 3 introduces the two main data sets used in this study. The first set of results are presented in section 4, which shows the economic impact of the blackout on labor. The impact on neonatal health and other neonatal outcomes is discussed in section 5. Possible explanations for low birth weights among the early exposure cohort are detailed in 6. There, I first present some explanations that do not fit the results, and then I use the linked birth data-Labor Force Survey to show the connection between birth weights and the blackout. Finally, section 7 concludes.

2 Background information

2.1 Birth weights determinants

Birth weights are an important determinant of future health and social outcomes. Low weight is "probably the single most important factor that affects neonatal mortality, in addition to being a significant determinant of post-neonatal infant mortality and of infant and childhood morbidity" (Kramer 1987). Low birth weight (defined as less than 2.5 kg, or 5.5 lb.) is associated with a host of growth deficiencies and mental problems (Ounsted et al., 1971; Hofvaner, 1982), as well as a higher rate of childhood death (McCormick, 1985; Pethybridge et al, 1974) and coronary heart disease (Barker, 1995). Moreover, low birth weight leaves permanent socioeconomic effects. Behrman and Rosenzweig (2004) use US data on twins to show that increasing birth weight increases adulthood height and educational attainment; Conley and Bennett (2000) find lower probabilities of graduation in a sample of US siblings among low birth weight children.

Because of its importance to long-term health, there is an active research agenda on the determinants of birth weights. One such determinant is maternal nutritional deficiency. In his comprehensive survey of the medical literature, Kramer (1987) showed that low levels of maternal nutrition and low maternal pre-

[5] The popular press and the general public are particularly fascinated by the idea. An interesting example came from the Planning Minister of Uganda who affirmed that Òpower blackouts were fueling a baby boomÓ in his country (BBC, 2009). To my knowledge, there is no empirical evidence in favor of this hypothesis, since most work on fertility has focused on permanent income [see discussion in Dehejia and Lleras Muney(2004)]. However, note Jensen and Oster (2010) and La Ferrara et al. (2008) for evidence that televisions reduce fertility. While the mechanisms they prefer rely on changes to the local culture, results are also consistent with the possibility that television changes the allocation of time devoted to domestic leisure, the mechanism that is likely behind the fertility result in this paper. See also Burlando (2009).

[6] More broadly, it fits within a burgeoning literature that examines the micro-economic effects of infrastructural development, including Pande and Duflo (2007), Dinkelman (2008), Gonzalez-Navarro and Quintana-Domeque(2010).

pregnancy weight can affect fetal growth and lead to prematurity and/or low birth weight. He concluded that maternal nutrition both before and during the pregnancy explains over 50% of cases of low birth weight in many developing countries. Randomized trials of food supplementation showed increases in birth weight among children whose mothers had low body mass indices (MacDonald et al, 1981; Mora et al, 1979; Habicht et al, 1974). Reductions in caloric intake affect the fetus directly, since fetal growth slows at any point during pregnancy if maternal nutrition is reduced (Harding et al, 2006).

The effects of the timing of the nutritional deficiency on birth weights is somewhat less understood. Nutritional studies mentioned above focused on pregnant women after the second trimester. Natural experiments (such as the one employed in this paper) can be used to study all trimesters. The Dutch famine of 1943 provided one such example: in that instance, lower birth weights were recorded for children exposed in their last trimester of gestation (Stein 1975). On the other hand, Almond and Mazumder (2008) find lower birth weights for children with *in utero* exposure to Ramadhan in the first month of pregnancy.

There is some evidence that, among certain women in developing countries, food intake during the first and second trimester plays an important role in fetal growth and birth weight. While fetal growth is still slow during this period, research on women in West Africa indicates that food is transformed into maternal fat deposit, and that this deposit is then used for fetal growth in the third trimester (Lawrence, 1987). Lack of stored fat can lead to intra-uterine growth retardation.

2.2 Blackouts in Africa

Although there are no existing statistics on the phenomenon, many countries in Africa suffer from tremendous power instability. Cities like Lagos in Nigeria are renown for constant power cuts. Other places where service has historically been considered reliable have been in the news for blackouts, including Addis Ababa, Nairobi, Dar es Salaam, and Johannesburg–all of which have suffered power outages lasting weeks if not months in the past five years. Such cities generally suffer from rolling blackouts, in which access to power is rationed but available for a few hours during the day or the week. This paper, on the other hand, considers a type of blackout that is protracted and without reprieve for weeks or months on end. Such cases are not uncommon, especially in rural areas. For instance, in the summer of 2008 local Tanzanian newspapers reported a 3 week long blackout in the Mtwara region on Tanzania. In Zanzibar itself, there was a new and even more serious blackout between the months of December 2009 and March 2010 (O'Connor, 2010). It is also possible to find accounts of protracted blackouts in areas that are at the margin of big cities suffering from rolling blackouts (BBC, 2010). In the former instances, protracted blackouts are often caused by unstable infrastructure that is prone to breaking and theft. Areas served by a single power line (as opposed to a grid of several lines connected to each other) are especially prone to incidents. In peri-urban areas, protracted blackouts might simply be the result of the allocation choices set by the utility company, which might choose to protect formal clients as opposed to informal ones. In either case, it is reasonable to expect that protracted power cutoffs will become more widespread as governments push for electrification in more marginal urban and rural areas.

2.3 The 2008 Zanzibar blackout

2.3.1 Unfolding of events

The Zanzibar blackout started on May 21, 2008 at approximately 10 p.m. and lasted until June 18, 2008.[7]. The cause was the rupture of the undersea cable that connects the Zanzibar island substation with the electricity generators on mainland Tanzania. Why the cable broke at that time is the subject of speculation, although it happened a few minutes before halftime during an important international soccer match–the Champion's League final that pitted Chelsea against Manchester United. The biggest soccer event of the year featured the two most followed teams on the island (*pace* Liverpool). Crowds gathered in traditional meeting places where home televisions were set up, and most televisions were tuned to the match in Moscow. It has been suggested, perhaps mischievously, that the staff at the utility company were among those watching the game. Without paying much attention to the aging machinery, they did not shut the system down during a power surge originating on the mainland. Even allowing for staff negligence, interviews with Zanzibar Electricity Corporation (ZECO) officials clearly point to underinvestment in maintenance as the ultimate culprit (Mzee Ally, 2008). The relaying system, designed and built under Norwegian financing, had never been upgraded in the forty years since it was installed, and had exceeded its expected lifetime.

It took just a few days before it became clear that the problem was serious, and the blackout was likely to be long (BBC, 2008). On June 3—two weeks into the power cut—a Norwegian technician arrived to assess the damage, propose a solution, and indicate a possible resumption date. The technician's assessment was the cause of much confusion: the morning after, one newspaper reported an estimated resumption of power in July (The Guardian, 2008), whereas another reported the date to be September (Citizen, 2008). In a radio address, the President of Zanzibar encouraged citizens to get used to candlelight dinners, which he admitted he found quite romantic. Disillusioned Zanzibaris believed that the situation would not improve before Ramadan in September.

On June 17th, the government announced the imminent restoration of power. The following day,

[7] Since Zanzibar is an important tourist destination, it is worth noting that this power outage stroke during the low season, a period where few visitors come and most resorts are closed. The same cannot be said for the blackout of 2009-2010, which hit the tourism sector hard (O'Connor 2010)

electricity was flowing.[8] The restoration took many people by surprise, since the government had been careful to play down expectations of a quick solution. The event was the longest recorded time without power in Zanzibar's history, and figure 1 makes clear that it was an unprecedented event.

2.3.2 Some impacts of the blackout

The lack of electricity affected daily life in a variety of ways. The most significant effects, on work and health, will be treated after section 3. Some other notable impacts of the blackout are listed here.

Leisure and time use For those households with a domestic power connection, lack of artificial lighting and television altered daily habits in significant ways. As I show in the companion paper, social interactions decreased in areas with high electricity coverage (Burlando, 2009). In particular, people reported returning home sooner than usual, and spending fewer evening hours out in the traditional *baraza* meeting places.

Generator use Use of petrol-run generators remained limited throughout the period. The price of generators shot up two- to ten-fold due to restricted supply, and remained high throughout. Moreover, running costs were also very high–reportedly in the order of 35-40 US dollars a day (BBC, 2008). As I will show, usage of generators had a limited impact on work hours, suggesting that people used them sparingly.

Food availability The blackout did not disrupt the supply chain of farm and animal products, and did not limit the availability of food. Food markets for dry and semi-perishable goods function largely without electricity or refrigeration. All wholesale trades of farm goods take place in a central market which is within three hours from any point on the island via frequent bus connections (which also serve to transport agricultural products). The supply chain for most locally produced goods is short and independent of electricity.[9]

Some suggestive empirical evidence against the presence of a blackout-induced food supply shock comes from the time series of weekly average wholesale prices for two widely consumed staples, cassava and potatoes (figure 2). The data come from a compilation of daily minimum and maximum trade prices collected daily by the Ministry of Agriculture from the wholesale market.[10] Disruptions in the food supply chain should cause price increases for these two products. The figure shows that while potato prices did increase both in magnitude and volatility during the blackout, such price increases were happening prior

[8] A limited number of rural areas reported a continuation of the blackout for a number of days after restoration. I was unable to obtain any information on these areas, other than the fact that they affected a small proportion of the population.

[9] Sales of some imported and processed goods such as icre-cream and premium meat cuts from mainland Tanzania and abroad did stop during this period. These premium products are not widely consumed by the population.

[10] These records are hand-written in dozens of notebooks and precariously stored in a locker in the market; several notebooks were missing at the time of my collection. Also, data was missing for products which were not in season at the time of the blackout.

to the onset of the power crisis. Furthermore, the price of cassava remained within the boundaries of the pre-blackout average.[11] The lack of price increases in cassava indicates that there was not an unusual change in the supply of this product. The price variation observed for potatoes could have been caused by supply shifts; however, a more likely explanation is that during the blackout there may have been a demand shift away from expensive starches (rice) toward cheaper sources of calories (potatoes).

Unfortunately, there are no written records of prices and quantities of traded fish, meat or milk. These highly perishable foods are also sold in open markets without any electricity or refrigeration. For meats and fish, whatever remains unsold at the end of the day is left overnight in freezers, and are generally de-thawed once more before they are sold or thrown away. Thus, even in normal times the life cycle of these products is very short, mostly starting and ending within the same day. During the blackout, freshness became more easily observable since there was no end-of-day refrigeration. Generally, prices became more sensitive to it. Lower average prices may have drawn more customers, but the narrow one-day shelf life may have reduced the quantities fishmongers were willing to purchase for resale. While it is unclear what the equilibrium outcome might have been in terms of prices and quantities, what is clear is that none of the perishable food markets shut down, there remained active consumption, and that all food types were available. The blackout did not break down supply of foods and nutrients.

3 Data

Post-blackout survey of households The first source of data is a household survey I collected five months after the blackout with a team of enumerators from the Government Statistics Agency that was specifically designed to gather information on the blackout. The sample consists of 366 randomly selected households in 19 villages and towns on the island of Zanzibar, selected from high, medium and low electricity coverage villages and neighborhoods. 12 survey locations are rural or semi-rural villages from the North, East, and South of the island, and have electricity coverage varying from 0 to 40% of the households. The remaining seven areas are urban and peri-urban neighborhoods of the main town, where between 70% and 100% of dwellings are connected to the grid. I collected the data over a one month period, beginning in November 2008.

Respondents were asked about their family structure, asset ownership, income levels, education, religious practices, as well as use of electricity in their own home and work. For each household, enumerators

[11] it is difficult to establish this relationship econometrically due to the missing data, although the statistical analysis largely confirms this finding. The analyses, not presented here but available upon request, regress the log of daily price on month and year fixed effects, a "treatment" dummy for prices collected between May 21 and June 18, 2008, a "control" dummy for a period for which there is available data in different years, and interaction between the year 2008 and both "treatment" and "control" dummies. Results are somewhat sensitive to the selection of control period.

obtained work and leisure hours by interviewing all adults (aged 15 and over) whenever possible. The questions about hours of work covered three periods: the month before, the month of, and five months after the blackout. To capture the range of activities carried out by all household members, we collected descriptions of each type of income-generating activity, and a personal assessment of the number of weekly hours spent doing each activity within each time period.

Panel A of table 1 shows some summary statistics for this data set. 20% of workers report using electricity at work, and over 80% report only one income generating activity. The average worker in the sample earns more money (around 20% more) and is better educated than the average Zanzibari, due to the oversampling in urban neighborhoods.

Mnazi Mmoja maternity ward records Statistics on birth weight come from the main maternity ward on the island at Mnazi Mmoja Hospital. Mnazi Mmoja is located in Zanzibar Town and has relatively modern equipment and qualified staff. The ward delivers 500-900 children per month, and caters mostly to the urban population–the hardest hit during the blackout. The ward keeps a delivery book that lists the name, town of provenance, number of prior pregnancies, age and admission date of expectant mothers. The book also includes some basic child characteristics, such as gender, weight, and additional medical notes associated with eventual delivery complications. In June of 2009, I photocopied and began to enter in a database all available delivery books from January 2007 until the end of May 2009, thus covering facility births prior to, during, and after the blackout. In total, I transcribed 20,027 births from this two and a half year period. Next, from the same records I identified the village of residence of the mother, and linked them with administrative wards (*shehia*). The identification of administrative areas was not always successful: some birth records were left blank, others had misspellings or used ambiguous physical markers (for instance, "by the baobab tree" identifies several neighborhoods and villages). In total, 16,959 observations from 156 wards had traceable community identifications. Finally, birth records were linked with average ward characteristics as described by the Labor Force Survey (LFS) of 2007. The nationally-representative survey inquired about labor habits of Zanzibaris, including sector of employment, type of employer, and monthly earnings levels. Other characteristics in the survey included education levels, ownership of domestic good assets, and some food consumption characteristics (number of meals per day, number of days with meals containing meat or fish). The surveys were conducted in 137 *shehias*, out of which 76 were successfully matched with the birth records. Thus, the matched birth records-labor force survey includes 13,112 observations.

Panel B of table 1 shows summary statistics for all births in the sample, and panel C for the matched births-LFS. There are minimal differences between the two. Mothers are, on average, 26 years old and have had two and a half pregnancies. The sex ratio is skewed in favor of boys, who represent 54% of all births.

Birth weights are generally quite low, averaging just about 3 kg (6.8 lb.). As the largest maternity ward, Mnazi Mmoja delivers an average of 166 children a week, or 715 a month, although this number trends upwards due to population growth.

4 Labor outcomes

This section shows that the blackout did lead to a significant decline in labor hours and earnings. The decline was large for a relatively small fraction of the population, but was also transitory. To show the impact on work I use surveys of workers reporting their labor hours before, during, and five months after the blackout. The 790 workers in the sample naturally divide into two categories, a group that (prior to the blackout) uses electricity at work and that was likely affected directly, and the rest who would have been affected at most indirectly.

4.1 Work during the blackout

Denoting by ele_work_i the dummy that takes the value of 1 if a person i works with electricity, the following first difference model shows the contemporaneous effect on work hours:

$$\Delta hours\_db_i = \alpha_1 + \alpha_2 ele\_work_i + X_i\beta + \mu_i \tag{1}$$

where $\Delta hours\_db_i$ is the difference of log weekly hours of work for person i during the blackout (May 21-June 18, 2008) relative to before (April 24-May 20), and the vector X_i includes individual labor and leisure shifters, such as household wealth, education, age, and size of the household. Table 2 reports the results from this regression. The first column excludes the work electricity dummy, the second excludes the controls, and the final reports the full regression. The controls in the first column have little or no predictive power. The second column shows that workers that use electricity lost 25% of their hours during the blackout. The coefficient on the constant identifies the blackout effect on those who do not use electricity was nil. Finally, none of the controls in column 3 has statistically significant coefficients other than the work electricity dummy, meaning that the change in hours did not systematically vary across the two groups along other non-treatment characteristics.

In table 3, I introduce several other possible explanatory variables of interest. In each column, I regress the dependent variable on the set of controls , the dummy for work electricity, a characteristic dummy, and the interaction between the characteristic dummy and work electricity. The characteristic dummy further divides the sample into additional groups. Because of the limited sample size, regressions have limited power, which affects the statistical significance of the results. Nonetheless, coefficient estimates are of interest. In the first two columns, I divide the sample between self employed and salaried workers (column 1), and between women and men (column 2). Self employed hours were more sensitive to the blackout than those of salaried employees: the estimated loss of hours for power users among employees was 15.4%. Among the self employed, the reduction was closer to 36%. Women also show larger elasticities than men (42% versus 14%). However, in neither regression the characteristic dummy is statistically significant, either with the interaction term or by itself (as reported by the F-statistic, and by separate regressions which are not shown).

Columns 4 and 5 address the possibility that blackout-caused changes in leisure could have affected labor supply. In particular, it is possible that lack of television and domestic electricity may have persuaded some to spend more time at work. Regression 4 includes a dummy for domestic electricity use and regression 5 for ownership of a television. There is significant collinearity between ownership of television/electricity and use of electricity at work, so the coefficients are all estimated with imprecision. T tests and joint F tests report no effect of these leisure shifters on the change in work hours. If there was a displacement of time from domestic leisure to the workplace, it was of a second order magnitude.

In columns 6 and 7, I focus on electrified workers. First, I show in column 6 that the use of generators did not make a big difference in the loss of hours. Among the treatment group, the estimated coefficients show a 30% reduction in hours for those without generators, while those with workplace generators lost less than half those hours. While sizeable, the difference is not statistically significant. The high running and maintenance costs limited the hours of utilization and the usefulness of these machines during the blackout.

More interestingly, column 7 shows significant differences between workers who used electricity for lighting, and workers who used it to power tools.[12] The first lost less than 10% of their hours during the blackout, whilst the latter lost an additional 35%. The result is not surprising: other than clearly being more dependent on power, the latter are also more specialized and so are less likely to find substitute tools or tasks.

Finally, column 8 interacts household wealth with the dummy electricity. I do not find any wealth effects, which might be surprising: it could be expected that potentially credit constrained workers cut fewer hours of work. Absence of income effects suggests that the blackout caused a labor demand shock rather than a labor supply shock.

Having established that electrified workers were severely affected by the blackout, I next explore the presence of other effects within the households of affected workers. In particular, I consider the possibility

[12] Tools include any type of electricity-run productive capital. This includes unusual goods such as fridges (which are rented out or used to store juice), and excluding non-productive goods like air conditioners.

that large-scale reallocations of labor within affected household might have disproportionately increased market or domestic work for women. This is relevant to the discussion on neonatal outcomes, because it is believed that increased and excessive work for pregnant women can lead to low birth weights (Kramer, 1981).

To check for this, table 4 regresses the change in labor hours $\Delta hours\_db_i$ for women on the change in hours of work for the rest of the household. Since women engage in both market activities and domestic chores, I consider both types separately and together. Columns 1 and 3 indicate that the change in own work hours is positively correlated with the change in other household members' hours. The OLS estimates, however, include responses to household level shocks, which are likely correlated with individual level shocks. To separate the two, columns 2 and 4 use work electricity dummies as instruments. The first dummy takes the value of 1 if the woman herself uses it at work. The second dummy indicates whether the woman lives in a household where there is at least one person (other than herself) who uses workplace electricity. The two coefficients in column 2 indicate that electrified women worked 42% fewer market hours, but the reduction was only 37% smaller if there were other household members who were similiarly affected. Column 4 finds small effects for domestic hours too. The amount of household work actually decreases for those women who have a relative who uses electricity at work, possibly because some affected workers seem to fill their lost hours by helping out at home. Column 5 sums up market and domestic hours: overall, women in affected households spent fewer hours working during the blackout.[13]

4.2 Work after the blackout

There are a number of reasons we could expect ripple effects to propagate well after the blackout, from persistence of the economic shock to inter-temporal responses of labor supply to the shock. Using information on work hours five months after, I find that there were no lasting effects of the blackout, thus confirming that the consequences of the power shutdown were temporary. To see that, I run the following regression on the sample of workers interviewed five months after the blackout:[14]

$$\Delta hours\_ab_i = \gamma \Delta hours\_db_i + t_i + \upsilon_i \tag{2}$$

where $\Delta hours\_ab_i$ is the log difference in hours of work five months after relative to the month prior to the blackout. Seasonality t_i is assumed to be correlated with the employment sector, but uncorrelated to the

[13] While all adult women in the household responded to the labor survey, only one woman per household (usually the head or spouse) responded to questions about domestic work and other time use. Thus, sample sizes in columns 1-2 are not the same as columns 3-5. Robustness tests for columns 1 and 2 were conducted using a restricted sample of working women who responded to the time use survey, with no differences in outcomes.

[14] The regression is derived from a reduced form lifecycle model spelled out in the prior versions of this paper (available from author upon request).

use of electricity within that sector. The coefficient γ indicates whether the blackout had lingering effects. If negative (positive), then households affected by the blackout increased (decreased) their work after the blackout.

Table 5 reports results from this regression, where t_i is approximated by employment sector dummies. In the first column we find that the coefficient γ is positive but insignificant. In the following columns, I instrument for the size of the shock. This is because regression (2) has a built-in positive correlation from the fact that we are using hours before the blackout both on the left hand side and the right hand side of the equation. From the previous section, we found two potentially useful instruments, work electricity and electric tools. In column 2, I show the IV results from using these two instruments. Again, the coefficient remains positive but insignificant. In the next column I use a set of instruments that better capture the size of the shock by directly including log hours of work during the blackout in the following first stage regression:

$$\begin{aligned}\Delta hours\_db_i = {} & \alpha_0 + \alpha_1 work\_ele_i + \alpha_2 hours\_d_i + \alpha_3 ele\_tools_i + \alpha_4 work\_ele_i * hours\_d_i \\ & + \alpha_5 ele\_tools_i * hours\_d_i + t_i + \upsilon_i\end{aligned} \tag{3}$$

The variable $hours\_d_i$ is correlated with the change in hours during the blackout, but does not enter into the change in hours after the blackout.

First stage results from regression (3) appear in column 3. The instruments have a strong predictive power, with an R2 of the excluded regressors equal to 0.64. I reject the assumption that instruments are weak. Moreover, the Sargan-Hansen J test has a Chi squared value of 4.46, and I fail to reject the hypothesis that the error is uncorrelated with the instruments.The coefficients largely move along the direction expected. When hours during the blackout are zero, we find that the change in hours is strongly negative for those who work with electricity tools (the coefficient is -2.75). As hours of work during the blackout increase, the decrease in the change in hours falls, and falls the fastest, again, for those who work with electric tools.

Using the predicted estimates from regression (3), column 4 estimates the second stage regression of the change in hours after relative to before the power outage, on predicted change in hours during relative to before. This time, the coefficient is negative, but remains very small, and very insignificant. Note that it has very little predictive power, with an R2 of 0.001.

What do these regressions tell us about the magnitude of the effects of the blackout? The power shutdown had a significant contemporaneous impact on labor hours for users of electricity, but not on those who do not use it. Moreover, the shock on labor completely dissipated within five months of the blackout. Thus, the blackout was the root cause of a temporary, and asymmetric, income shock. An estimate of the size of the income shock can be found in table 6, which shows the estimated earnings losses for workers (first column) and for households, where the latte was found by adding up work hours of all household members. The results indicate very high losses for close to 10% of households, small to moderate losses for an additional 30%, and little effects for the remaining 72% of families.

5 Neonatal outcomes

To establish the relationship between the blackout and neonatal outcomes, I run a regression on outcome y_{it} for child i born in day t using a set of regressors that measure the timing of in-utero exposure to the blackout:

$$y_{it} = \alpha_0 + X_{it}\beta + \sum_{j=1}^{9} \gamma_j month_{jit} + t + \epsilon_{it} \tag{4}$$

In this regression, I assume that outcomes are determined by child and mother characteristics X_{it}, a series of time controls t (quarter and year of birth dummies) that capture secular and seasonal changes in the time series, and a set of exposure dummies $month_{jit}$ that indicate whether the child was exposed to the blackout during month j of gestation. The controls include the information available in the birth records: age, age squared, and number of prior pregnancies of the mother; and whether the child is a girl or a twin. Unfortunately, I do not observe the last menstrual period (LMP), which provides an approximate date of conception. I assume throughout that a child born at a certain date was conceived 38 weeks prior to birth.[15]

I also make use of a refinement in the above regression that pools together some children into an "early exposure" cohort, where the dummy $early\_exposure_{it}$ will be explained in the following section:

$$y_{it} = \alpha_0 + X_{it}\beta + \alpha_1 early\_exposure_{it} + \sum_{j=1}^{9} \gamma_j month_{jit} + t + \epsilon_{it} \tag{5}$$

5.1 Birth weights

Results from regression (4) for birth weights are found in table 7, column 1. Children exposed in the first month of gestation have significantly lower birth weights on average, by 45 grams (1.6 oz). No other cohort of children reports lower birth weights, although coefficients are negative for months 2, 3 and 5. In column 2, I include an additional dummy, for children who were conceived during and up to 30 days after the power cut. This "month 0" cohort indeed had significantly lower birth weights. There are two explanations. First,

[15] The lack of actual gestation data is not particularly limiting. For instance, using both gestation and predicted gestation in a sample of Michigan Arabs, Almond and Mazumder (2010) do not find significant differences in the estimated coefficients of Ramadan exposure.

the negative economic effects of the blackout could have continued post blackout, so fetal exposure to the economic shock would include later cohorts of children. Second, since as mentioned earlier maternal weight at conception is an important determinant of birth weight, low weights could be recorded from children of mothers who suffered blackout-induced weight losses, even if incomes recovered immediately with power restoration. Note further that the dummy on month two is now more negative and statistically significant, while the dummy for month one is not (although a F-test fails to reject the possibility that the two are actually equal in magnitude). This suggests that significance from the coefficients on these two months is coming from children exposed both in the first and second month of the pregnancy. In column 3, I explore this further by allowing differential effects for children exposed in the first six weeks from conception. The coefficient estimates for this cohort are more negative and indicate a reduction of 100 grams. The "month 0 cohort" also remains negative and significant, with a coefficient of 86. Since the two estimates are close and statistically indistinguishable between them, in the following column I pool both group into one "early exposure" cohort, so that the regression is now (5). For the remainder of the paper, I will estimate treatment effects for this pooled cohort.[16] This regression shows that children conceived within six weeks and up to 30 days from the power outage weighed 78 grams less than expected. Interestingly, this regression also shows significant and sizable reductions for children exposed to the blackout in the fifth month of gestation. These results are strikingly similar to those found in Almond and Mazumder (2008) for Muslim children exposed to Ramadan while in utero, although later regressions will show that only the early exposure cohort can reasonably be deduced to have been impacted by the blackout.

Table 8 provides some robustness tests to regression (5). Column 1 includes a dummy for those children who were born in the 30 days prior to the onset of the blackout, and who were therefore unexposed to it. As expected, this cohort did not have significantly different birth weights. In the following two columns, I vary the seasonality controls, first by adding quadratic time trends (column 2) and then by replacing quarter of birth dummies with month of birth dummies. The estimates for the group exposed early in gestation period are unaffected, while the estimated coefficient for those exposed in the fifth month of gestation falls and becomes insignificant.

In column 4 I run the most demanding robustness check. I restrict the sample to those observations that were matched with the Labor Force Survey, and include month, year and *shehia* fixed effects. The inclusion of *shehia* fixed effects had no impact. This reassures us that the estimated coefficient on early exposure is not caused by unobserved shifts in demand for perinatal care from areas more susceptible to lower birth weights. Moreover, since the estimated effects for the restricted sample match those from the full sample, in later sections I will make use of this restricted sample to augment the birth record dataset

[16] Magnitudes and significance of coefficients do not vary in regressions that keep the two groups separate.

and explore transmission mechanisms.

Finally, in column 5 and 6 I use an exposure variable similar to one found in Almond and Mazumder. Rather than using a month of exposure dummy, I calculate the number of days in the gestation month that a child was exposed to the blackout, and report the estimated coefficients for months 0 through 2, and month 5. The interpretation of the coefficients is different: the estimates indicate the average weight loss for each day of exposure to the blackout in the specific month (with the exception for month 0, which indicates the in-utero exposure to each of the 30 post-blackout days). The effects mimic what was found in columns 1 and 2 of table 7, and are consistent with Almond and Mazumer.

Average declines in birth weights are not notable by themselves. What really matters is the distribution of those declines, and the incidence of Low Birth Weight (LBW). In that respect, the estimated birth weight losses are very notable here because they disproportionately affected the bottom of the birth weight distribution. I show this in figure 3, which shows coefficient estimates of $early\_exposure_{it}$ for quantile regressions at the 8, 16, 33, 50, 66 and 83 percentiles of weight. Lower birth weights are registered throughout the weight distribution with the largest drop registered at the 8th percentile, where birth weights average around 2 kg and where they were 150 grams less than expected for the affected early pregnancy cohort. The first two columns of table 9 focus directly on LBW. The probability of LBW was 11% higher for children exposed early to the blackout, but there were no LBW effects for those exposed in their fifth month of gestation.

5.2 Sex ratio and selected fertility

Next, I provide some additional results from two other observable outcomes, the sex ratio and the number of deliveries, both of which are compiled with a weekly frequency (for a total sample size of 107). The first is of interest because other studies have pointed out the differential effects of nutrition shocks during gestation for girls and boys. (Roseboom et al 2001, Cameron 2004, Mathews et al 2008). The second is relevant because conceptions could have increased during the blackout. Since the blackout increased leisure time and reduced work, the instantaneous opportunity cost of having children temporarily fell during the blackout, and as a consequence both planned and unplanned pregnancies may have taken place.[17] These outcomes are relevant for the cohort of children conceived during the blackout, so I restrict the regression by excluding exposure month dummies and focus on the early exposure cohort alone.

Column 3 of table 9 shows that there were no changes in the sex ratio. Column 4 also suggests that the early exposure cohort was larger by around 19 weekly births than expected (11% of the mean number

[17]One of the most widespread urban legends regarding the 1979 New York City blackout was that it caused a jump in birth rates nine months later.

of weekly births) . While this result is not statistically significant, it becomes so once quarter of birth are substituted with month dummies (column 5). The larger cohort size could have been a direct consequence of the blackout, provided that the effect was temporary. Table 10 provides some evidence in favor of this hypothesis: the cohorts of children exposed to the blackout in the second and third trimester were no more numerous than expected, as was the group conceived after one month from the blackout. The unexplained increase in the number of births affects only the "early exposure" group, which includes those conceived during or immediately after the power cutoff.

One consequence of this temporary surge in fertility is that there could have been adverse selection into pregnancy, affecting women who are more likely to deliver smaller babies. Moreover, some of these induced pregnancies could have been unwanted, and subject to lower in-utero investment from the mother. For instance, first pregnancies generally deliver smaller babies. Similarly, women on the right side of the fertility distribution (having had more than three children) could conceivably be more likely to have an unwanted pregnancy. On the other hand, women with less than two prior pregnancies are likely to have excess fertility. Similar arguments are applicable to age groups.

I show in table 11 that this selection into pregnancy did not drive birth weights. The left side of table breaks down the weekly number of births by observable maternal characteristics likely correlated with lower birth weights–here, number of prior pregnancies and age profile. It regresses the weekly number of women fitting the profile on time controls and the early exposure dummy, whose coefficient is reported in the second column. To check for the possibility that a particular age or pregnancy profile of women is driving birth weights, the right side of the table breaks down birth weights by the profile of the mother by regressing (4) with an interaction term:

$$
\begin{aligned}
birthweight_{it} = \alpha_0 + X_{it}\beta + \sum_{j=2}^{9} \gamma_j month_{ij} + \alpha_1 early\_exposure_{it} \\
+ \alpha_2 mother\_type_{it} + \alpha_3 early\_exposure_{it} * mother\_type_{it} + t + \epsilon_{it}
\end{aligned}
$$

where $mother\_type_{it}$ is a dichotomous variable for number of pregnancies and age profiles. Thus, coefficients on the interaction term and on $early\_exposure_{it}$ are intended as the treatment effect on the birth weight of children whose mothers respectively do and do not fit the given profile.

The left hand of the table shows that first pregnancies and women with two prior gravidities were more likely to conceive during this period. Finally, no age profile is overrepresented in the sample, although it is notable the large coefficient estimated for underage girls.

The right hand side checks the effect of adverse selection on birth weights. If teen pregnancy or first birth were driving the results, the coefficient on $early\_exposure_{it}$ should be zero, and the coefficient on $early\_exposure_{it} * mother\_type_{it}$ should be significant and negative for teenage girls or first pregnancy groups. For all specifications considered, $early\_exposure_{it}$ remains negative and close to the 70-80 range, while the interaction term for most regressions is small and statistically insignificant.

6 Causes of lower birth weights

6.1 Disruptions to health services

The results presented so far show a temporary drop in birth weight at a maternity clinic four months and seven to nine months after a major blackout in Zanzibar. In this section, I explain possible reasons for this drop. While the blackout could have the underlying cause, it is important to rule out alternative explanations. The first possibility is that the recorded weight loss is an artifact of the data, caused by a temporary and unrelated change in the composition of health-seeking pregnant women coming to the hospital. As shown in table 9, there was no consequential change in composition of *shehias* where women resided. However, there might be selection of mothers at the hospital among other dimensions, such as shocks to health services that are unrelated to the blackout. Based on discussions with health care professionals in multiple clinics and at the ministry of health, I found no evidence of other shocks in the months following the blackout. There were no more blackouts, no obvious policy changes in the way hospitals were run, no hospital closures, and no other major upheavals.

A possibility is that the blackout itself caused disruptions to health services that translated into temporary changes in the composition of women several months after the fact. Note that direct disruptions to the maternity ward cannot be considered a valid explanation: in the presence of those, we should expect fewer births and lower weights in the months during and immediately following power resumption. We should also see these numbers returning to their normal average over time as disruptions were fixed and quality of care improved. This is not the pattern found.

On the other hand, disruptions to ante-natal care (ANC) could have had a delayed effect. ANC clinics are widely attended by pregnant women: the Demographic and Health Survey (DHS) reported that 97% of Zanzibari women sought ante-natal care during their last pregnancy (NBS, 2005). They provide a service which in itself could affect child health and birth weights. Disruptions to counseling and medicines could have led to smaller babies and to fewer hospital deliveries. Again, it is unlikely that ANC failures drove any of the results found here, at least for two reasons. First, ANC clinics are very low-tech. Visits generally take place early in the morning, and neither the medical visit nor the standard tests (weight, blood, anemia, malaria) require electricity. Second, first visits to the ANC clinic generally happen at a later stage of pregnancy. DHS reports that only 12.4% of pregnant women visit before their fourth month, and the median woman goes when she is 5.6 months pregnant. If there was a "ANC effect", we should have seen lower birth weights for women in their second trimester at the time of the blackout. This could explain the drop among newborns exposed in the fifth month of gestation, but not those exposed early.

6.2 World food prices

One important event that was contemporaneous to the blackout and its aftermath was a worldwide increase in cereal prices. Between March and October, 2008, world prices of cereals were at historical highs. It is feasible that high world food prices were reducing food intake independently of the blackout. To check this possibility, I include the world price of the most important commodity, maize, averaged over a certain length of period into the birth weight regression (5). Prices are averaged over the entire gestation period (column 1), over the whole year prior to birth (column 2), over each trimester of gestation(columns 3, 4 and 5). In column 6, all of the above averages are included. Overall, birth weights are negatively correlated with maize prices over the gestation period, with the exception for the second trimester, but they are never close to statistical significance here. Moreover, the coefficient on $early\_exposure$ remains negative, significant, and close to the original estimate. The effect on fifth month also remains strongly negative and of similar magnitude of that found in table 12, although estimates are insignificant. Other regressions that included other price vectors (such as prices for rice) lead to similar results: price movements cannot account for the dip observed in the early exposure cohort.

6.3 Income shock

The main hypothesis of this paper is that the reduction in birth weights was a consequence of the income shock described in section 4. If the income shock was the cause, then lower birth weights should be concentrated among residents of wards most directly exposed to the blackout. To check for this, I use the merged birth records-labor force survey to construct *shehia*-specific indicators that are correlated with exposure to the blackout. Note that this restricts the sample of births significantly, although it does not bias it. There are two potential indicators of interest. The first, $work\_ele_v$, is the proportion of *shehia* workers employed in jobs that use electricity. While this information is not directly available from the LFS, I use the sector of employment to proxy for employment in "electrified" jobs. In particular, I assume that *shehias* with large numbers of workers in specialized occupations—managers, professionals, technicians, clerks, plant and machine operators–were more likely to be impacted by the blackout, whereas other job categories—sales, crafts, domestic services, fishing and farming, and other "elementary occupations" —were not. As a robustness check, I also employ the proportion of households having and electric hookup in the home, dom_ele_v. With this information, I run the following specification:

$$birthweight_{ivt} = \alpha_0 + \tilde{X}_{ivt}\tilde{\beta} + \sum_{j=2}^{9} \gamma_j month_{ivj} + \alpha_1 early\_exposure_{ivt} \tag{6}$$

$$+ \alpha_2 electricity_v + \alpha_3 early\_exposure_{ivt} * electricity_v + t + \epsilon_{ivt}$$

where $\tilde{X}_{ivt}$ is an expanded control set that include aggregate *shehia*-level controls that include average education of households heads, average asset holdings, and average monthly earnings, and $electricity_v$ is the proxy of maternal exposure to the blackout at home or at work.

A basic prediction for the first measure of electricity would involve $\alpha_2 = 0$ and $\alpha_3 < 0$: areas whose jobs were harder hit should report lower birth weights. However, the employment composition of a ward is likely to be correlated with other social and economic variables that allow households to smooth out shocks. In particular, areas with high concentration of professional workers are likely to be wealthy, and better able to smooth a temporary decline in earnings. Thus, I also test the hypothesis that the relationship is concave, and that there are other relevant interactions with income.

The interpretation of the alpha coefficients when exposure is proxied by the levels of domestic electricity coverage is less straightforward. Provided that domestic and work electricity are correlated, the coefficients in those estimates could reflect the income shock. Controlling for this income effect, there should have been no impact on food preparation and consumption, given that most households have access to cooking technologies that are independent of electricity. Thus, any effect of domestic electricity is likely to capture maternal stress. If maternal stress caused by lack of electricity at home was the driving factor, then the coefficient $\alpha_3 < 0$. In this case, we should not expect a U-shaped relationship, since stress is unlikely to be diminished by the wealth of the neighborhood.

Table 13 provides the main results for work electricity. Column 1 looks directly at specification (8), excluding ward-level average gross household earnings. The interaction coefficient is positive and highly imprecise. The hypothesis that birth weights and exposure are linearly related is therefore rejected. Column (2) through (6) allow for $work\_ele_v$ to enter the equation in a quadratic. In column (2), the linear and quadratic interaction terms are jointly significant, and account for the entire difference in birth weight with other birth cohorts. The U-shaped function indicates that the drop in weights is concentrated among those areas that have around 20-25% of households working in "electrified"sectors. One reason for this U-shaped response is that wards with high levels of employment in professional sectors are wealthier and, therefore, better able to smooth out the shock. To check for this, columns (4) through (6) include gross earnings interacted with $post\_blackout_t$. Gross earnings are positively correlated with birth weights, and as expected they reduce the effect of the blackout. However, the inclusion of the interaction is not enough to eliminate the concavity—if anything, it makes it more significant.[18]

Table 14 provides results for domestic electricity use, after controlling for the main effect through work electricity. For the cohort exposed early, across all specifications the evidence is that there are no additional effects of domestic electricity. In column (3) and (4), I interact domestic electricity with the dummy for exposure in 5th month. This time, the coefficient on the interaction term is negative, large, and statistically significant. Moreover, the interaction explains most of the effect found in children exposed in the fifth month. Finally, note that the quadratic interaction terms in (4) are not statistically important: we do not find the U-shape relationship between weights and income shock exposure. This suggests that the mechanisms are likely to be different for the two cohorts of affected children.

7 Conclusion

I use a month-long blackout that unexpectedly hit the Indian Ocean island of Zanzibar, Tanzania, in May 2008 to measure the effects of power instability on earnings and on birth weights. Using a household survey collected during field work, I find that the blackout caused significant income losses among those households who use electricity at work, but had no additional effect on other households' earnings. Moreover, the effect of the shock was short-lived, with labor hours and earnings returning to normal within five months.

I also use records from a government hospital to show that those children who were conceived during or shortly after, those exposed during the first six weeks of gestation, and those exposed in the fifth month of gestation had lower birth weights on average than expected. Moreover, among those exposed early, there was a marked increase in probability of Low Birth Weight.

While several explanations exist that might explain the drop in average weights, the data is most consistent with a reduction in caloric intake by the affected expectant mothers. Such a drop might be explained by a blackout-related income shock. I show that birth weights were lowest among those who were born from parents residing in wards with a significant concentration of workers in electrified sectors. Moreover, there is some evidence that among the cohort of children exposed in the fifth month of pregnancy, the driving factor to lower weights was not the income shock, but maternal stress.

[18] a more flexible specification that breaks $work\_ele_v$ into quintiles leads to the same outcome.

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[1]

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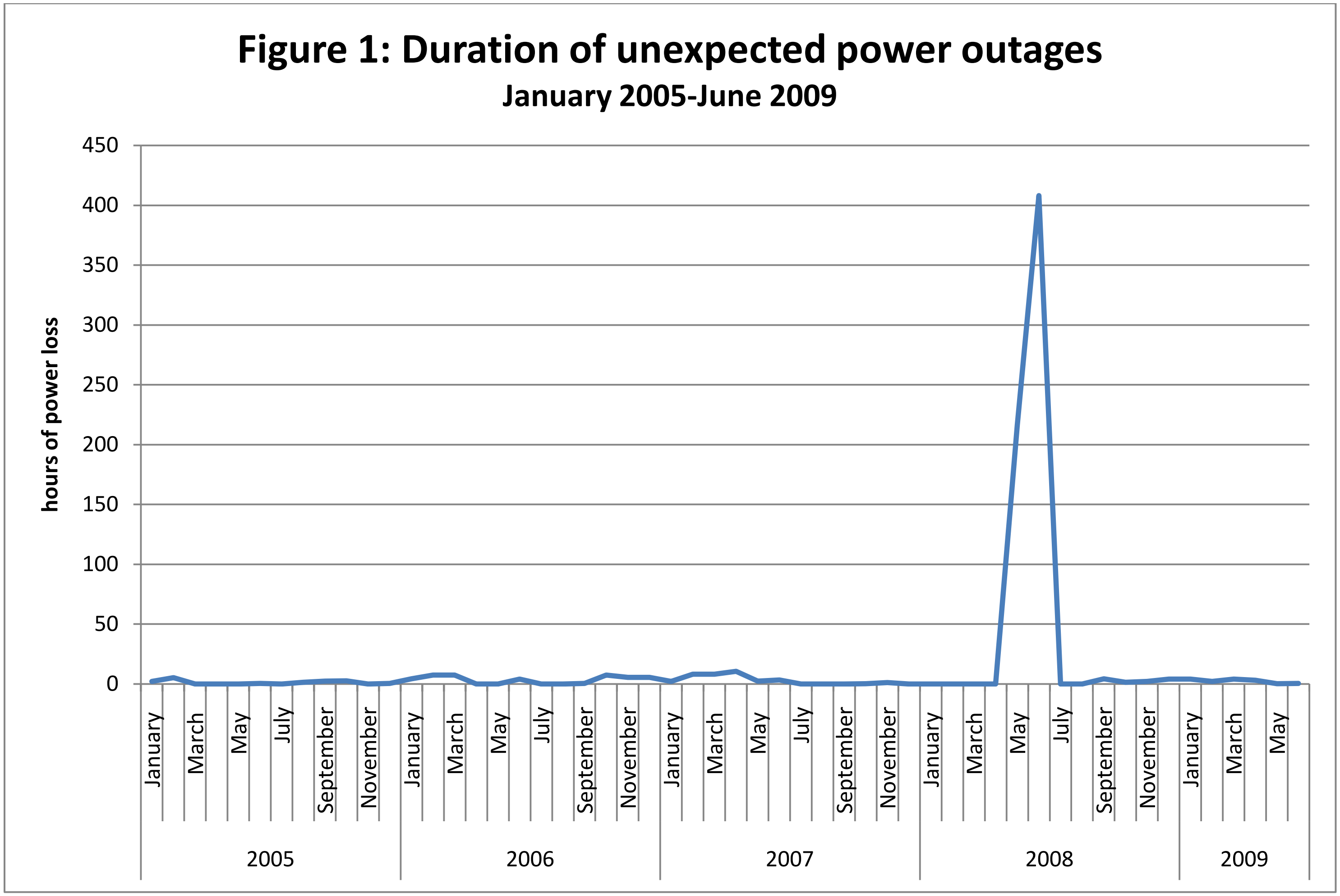
Figure 1: Duration of unexpected power outages
January 2005-June 2009
hours of power loss
450
400
350
300
250
200
150
100
50
0
January
March
May
July
September
November
2005
2006
2007
2008
2009

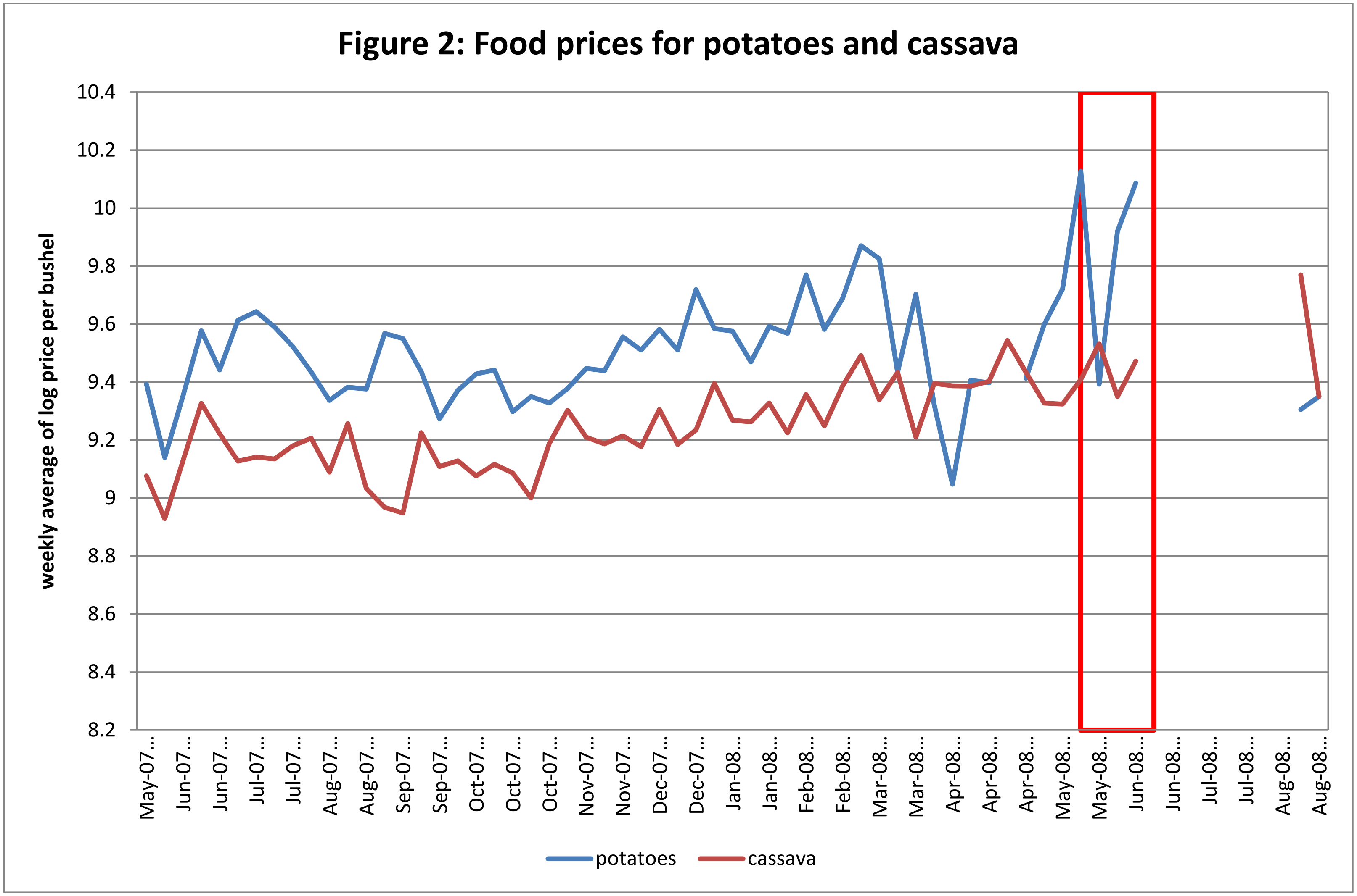

Figure 2: Food prices for potatoes and cassava
weekly average of log price per bushel
10.4
10.2
10
9.8
9.6
9.4
9.2
9
8.8
8.6
8.4
8.2
May-07...
Jun-07...
Jun-07...
Jul-07...
Jul-07...
Aug-07...
Aug-07...
Sep-07...
Sep-07...
Oct-07...
Oct-07...
Oct-07...
Nov-07...
Nov-07...
Dec-07...
Dec-07...
Jan-08...
Jan-08...
Feb-08...
Feb-08...
Mar-08...
Mar-08...
Apr-08...
Apr-08...
Apr-08...
May-08...
May-08...
Jun-08...
Jun-08...
Jul-08...
Jul-08...
Aug-08...
Aug-08...
potatoes
cassava

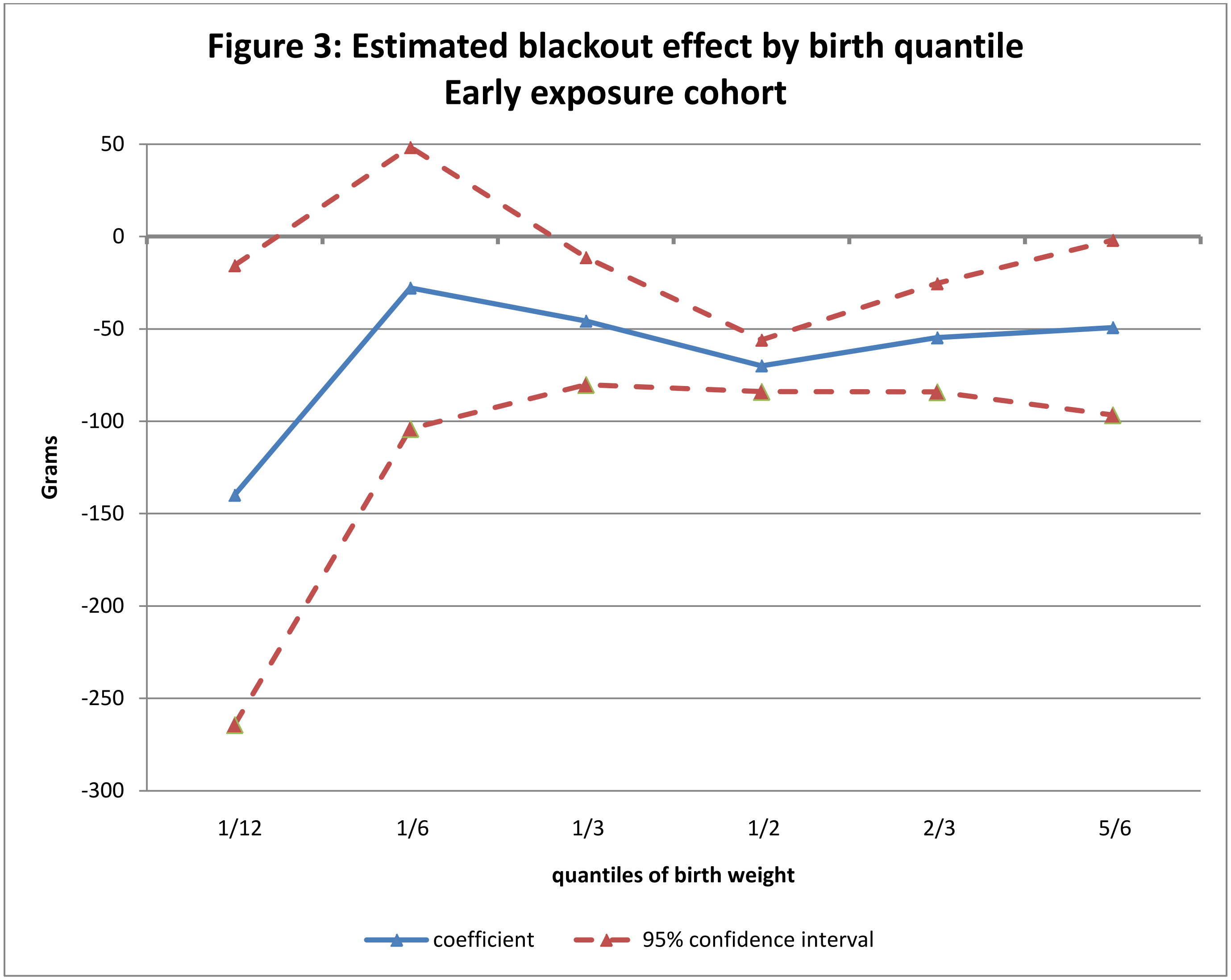
Figure 3: Estimated blackout effect by birth quantile
Early exposure cohort
50
0
-50
-100
-150
-200
-250
-300
Grams
1/12
1/6
1/3
1/2
2/3
5/6
quantiles of birth weight
coefficient
95% confidence interval

Table 1: Summary statistics

Variables	Summary statistic	St. Dev.
Panel A: Worker Characteristics		
proportion using electricity	0.19	0.39
number of activities	1.21	0.43
earnings (Tanzanian Shillings)	61,518.50	58,942.41
earnings premium of electricity users	1.52	
workers in hhld	2.43	1.19
education (years)	7.57	4.47
age	38.23	13.75
size of hhld	6.14	2.56
number of workers	790	
number of working age adults	1,164	
number of households	366	
Panel B: Births at Mnazi Mmoja Hospital		
	Full sample	
age of mother	26.57	6.62
number of pregnancies (gravida)	2.54	2.58
birth weight (kg)	3.080	0.67
number of weekly births	166.891	42.34
number of monthly births	715.25	174.7
number of weeks in sample	120	
number of births in sample	20,027	
Panel C: Matched births-Labor Force Survey		
age of mother	26.6	6.59
number of pregnancies (gravida)	2.81	2.33
birth weight (kg)	3.079	0.69
employment in "electrified" sectors	0.350	0.172
percentage connected to grid at home	0.563	0.281
number of births in sample	11,973	

Table 2: Log weekly hours of work before and during blackout

Dependent variable: $\Delta hours\_db$	(1)	(2)	(3)
Work electricity		-0.251***	-0.262***
		(0.078)	(0.080)
Domestic electricity	-0.033		0.011
	(0.058)		(0.056)
Female	-0.034		-0.033
	(0.033)		(0.032)
Gross monthly earnings	0.076		0.155**
('000,000 of Tsh)	(0.072)		(0.076)
Gross earnings^2/100	-1.644		-5.125*
	(2.671)		(2.694)
Age	-0.003		-0.001
	(0.004)		(0.004)
Age squared	0.003		0.001
	(0.005)		(0.005)
Education	-0.004		-0.002
	(0.003)		(0.003)
Household size	0.005		0.002
	(0.004)		(0.004)
Assets	-0.012		-0.006
	(0.011)		(0.010)
Constant	-0.001	-0.013	-0.034
	(0.102)	(0.012)	(0.107)
Observations	782	790	782
R^2	0.016	0.052	0.062

Observations for regression (1) are individual log of weekly hours measured before and during the blackout. Observations for regressions (2) and (3) are differences in log hours. Title of each column is dependent variable. Standard errors clustered at the household level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Change in log weekly hours: Heterogeneous effects of blackout

Dep. Var: $\Delta hours\_db$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Characteristic:	baseline	self employed	female	domestic elect.	television	generator	electric tools	wealth
work electricity	-0.250***	-0.149*	-0.144*	-0.256	-0.208	-0.307***	-0.080**	-0.232***
	(0.079)	(0.080)	(0.074)	(0.180)	(0.130)	(0.105)	(0.039)	(0.089)
characteristic dummy		-0.017	-0.008	0.021	-0.057	0.189	-0.353**	0.000
		(0.023)	(0.020)	(0.060)	(0.065)	(0.131)	(0.142)	(0.004)
characteristic dummy*work ele		-0.200	-0.264	0.006	-0.057		0.000	-0.009
		(0.141)	(0.173)	(0.196)	(0.157)		(0.000)	(0.025)
Observations	782	782	782	782	782	782	782	782
R^2	0.054	0.066	0.072	0.054	0.056	0.062	0.085	0.055
F test dummy		1.266	1.492	0.0918	0.667			0.122
Prob > F		0.283	0.226	0.912	0.514			0.885

Full sample regressions on first difference of log weekly hours of work during blackout relative to before. Title of each column indicates the characteristic dummy included in the dependent variable set. Controls include monthly earnings, age and age squared, education, and size of household. Robust standard errors in parenthesis, clustered at the household level. F statistics report the probability of joint significance for dummy and dummy*work electricity coefficients. Baseline reports the coefficient on work electricity from column 3 in table 3.

Self employed is defined as person who works in farming, fishing, or has own business/microbusiness.

Those who report using electricity at work but without using electric tools use electricity for lighting purposes only.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Change in female work hours during blackout

		(1)	(2)	(3)	(4)	(5)
	log differences in:	work	work	domestic	domestic	all work hours
		$\Delta hours\_db$	$\Delta hours\_db$	Δdom_db	Δdom_db	
	$\Delta hours\_db$	0.378		0.045		
	other hhld members	(0.261)		(0.052)		
work electricity:	own		-0.420**		-0.017	-0.477**
			(0.198)		(0.061)	(0.193)
	other hhld workers		0.050**		-0.057	-0.024
			(0.024)		(0.069)	(0.080)
	Observations	345	345	337	337	337
	R^2	0.148	0.107	0.022	0.028	0.112

Observations in all regressions exclude males. Columns (1) and (2) include all employed women, column (3)-(5) include employed and non-employed women who answered the time use survey. Column titles indicate the dependent variable, measured as the log difference in hours of market/domestic work during blackout relative to before. Column (5) dependent variable is the sum of the two types of activity. $\Delta hours\_db$ is the sum of the changes in market work hours for all the other workers in the household.

Controls include age and age squared, education, wealth and size of hosehold. Regressions (1) and (2) include earnings controls.

Standard errors are clustered at the village level.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Blackout effects on labor hours five months after

	(1)	(2)	(3)	(4)
	OLS	IV	First stage	IV
Dependent variable:	$\Delta hours\_ab$	$\Delta hours\_ab$	$\Delta hours\_db$	$\Delta hours\_ab$
$\Delta hours\_db$	0.109	0.060		-0.014
	(0.083)	(0.047)		(0.023)
work electricity			-0.353	
			(0.492)	
electric tools			-2.750***	
			(0.501)	
$hours\_d$			0.103**	
			(0.048)	
work electricity*$hours\_d$			0.074	
			(0.133)	
electric tools*$hours\_d$			0.696***	
			(0.135)	
Observations	790	790	790	790
R^2	0.066	0.056	0.641	0.001

Title column indicates dependent variable. In column 1, 2, and 4, the dependent variable is the difference in log weekly hours of work after the blackout relative to before. Instuments in (2) are work electricity and electric tools. In column 3, it the difference in log weekly hours during relative to before. All standard errors clustered at household level. All regressions include work sector dummies.
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Magnitude of losses in earnings during the blackout, urban sample

Magnitude of Loss	Proportion of workers	Proportion of households
Over 50%	5.49%	8.47%
20-49%	3.72%	0.66%
10-19%	2.34%	12.77%
4-9%	0.34%	6.29%
Gain or no loss	88.17%	71.81%

Proportion weighted by population using census weights.

Table 7: Baseline: Childbirth weight in gr. by month of predicted exposure to blackout

Birth weight in grams	(1) baseline	(2) including post-blackout cohort	(3) including post-blackout and first 6 weeks cohorts	(4) pooled pre and early pregnancy cohort
Predicted exposure in:				
Month prior to pregnancy		-72.3***	-85.7***	
		(25.1)	(23.1)	
First 6 weeks			-100.3**	
			(50.3)	
Early exposure				-77.6***
				(21.9)
Month 1	-45.2*	-16.9		
	(23.9)	(26.0)		
Month 2	-1.7	-56.6*	1.9	-9.6
	(26.6)	(33.9)	(46.7)	(24.2)
Month 3	-17.8	14.1	-36.0	-24.1
	(35.6)	(37.9)	(47.0)	(34.1)
Month 4	40.7	9.7	49.3	40.7
	(36.0)	(38.4)	(44.7)	(35.0)
Month 5	-61.5	-40.9	-73.2*	-65.6*
	(37.7)	(38.7)	(43.1)	(37.0)
Month 6	15.6	-5.1	20.7	15.2
	(35.2)	(36.3)	(39.3)	(34.8)
Month 7	11.6	26.4	6.4	10.7
	(34.0)	(34.6)	(36.6)	(33.7)
Month 8	7.5	-5.7	7.2	4.9
	(31.7)	(32.2)	(33.1)	(31.6)
Month 9	4.5	13.4	6.2	7.1
	(27.5)	(27.8)	(28.2)	(27.5)
Age	21.5***	21.4***	21.4***	21.4***
	(5.9)	(5.9)	(5.9)	(5.9)
Age squared	-37.8***	-37.7***	-37.6***	-37.7***
	(10.7)	(10.7)	(10.7)	(10.7)
Total pregnancies	23.0***	22.9***	22.9***	22.9***
	(3.9)	(3.9)	(3.9)	(3.9)
Twins	-761.8***	-763.1***	-764.3***	-764.3***
	(28.8)	(28.8)	(28.8)	(28.8)
Female	-107.5***	-107.6***	-107.8***	-107.7***
	(9.6)	(9.6)	(9.6)	(9.6)
First pregnancy	-94.9***	-95.0***	-94.8***	-94.8***
	(14.4)	(14.4)	(14.4)	(14.4)
Constant	2,781.5***	2,787.5***	2,789.3***	2,787.4***
	(85.6)	(85.6)	(85.5)	(85.5)
Observations	18195	18195	18195	18195
R^2	0.073	0.073	0.073	0.073

Birth weight measured in grams. P-value of F test of equality between pre-pregnancy and early pregnancy coefficients in (3): 0.78.
All regressions include quarter of birth and year fixed effects. Heteroskedasticity-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Robustness tests

Birth weight in grams	(1) including unexposed cohort	(2) quadratic time trends	(3) month & year f.e.	(4) month & year f.e. shehia f.e.	(5) number of exposed days	(6) number of exposed days
Pre and early pregnancy	-73.6***	-61.6***	-77.0***	-72.3**		
	(22.2)	(17.9)	(23.1)	(31.5)		
Month 5	-72.7*	-65.5*	-59.8	-67.5**		
	(37.5)	(34.5)	(37.8)	(33.5)		
Not exposed	-37.8					
	(28.9)					
Days exposed in:						
Month 0						-4.2***
						(1.2)
Month 1					-1.8	-2.7**
					(1.2)	(1.2)
Month 2					-2.0*	-3.7***
					(1.1)	(1.3)
Month 5					-2.6*	-2.7**
					(1.4)	(1.4)
Observations	18195	18195	18238	12004	18195	18195
R^2	0.073	0.074	0.074	0.082	0.073	0.074

Regressions (1)-(4) include predicted exposure month dummies. Regressions (5)-(6) inlcude number of predicted exposure days for each exposure month. All regressisons include controls from table 2. quarter and year of birth dummies in regresisons (1), (5), (6). Heteroskedasticity-robust standard errors reported for all columns. Errors clustered at shehia level in column (4).

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Other neonatal outcomes

Predicted exposure to blackout	(1) low birth weight dummy	(2)	(3) male/female sex ratio	(4) number of births	(5)
	logit	OLS			
Early exposure	0.217*	0.017*	-0.068	18.722	19.341*
	(0.120)	(0.010)	(0.080)	(12.213)	(11.555)
Month 5	-0.141	-0.009			
	(0.191)	(0.015)			
Average dep. var.	0.104		1.17	173	
Observations	19636	19636	107	107	107
R^2		0.054	0.169	0.596	0.683

Regression 1-2: dependent variable is a dummy for birth weights less than 2.5 Kg. Controls include month of exposure to blackout, maternal age, age squared, number of prior pregnancies, dummy for first pregnancy, and dummy for twin or girl baby. Regressions (3)-(5) are on birth records aggregated by week of birth. Controls for (3) and (4) include quarter and year of birth fixed effects and quadratic time trends. Controls for (5) include month and year of birth and quadratic time trends. Heteroskedasticity-robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Number of weekly births by period of blackout exposure during pregnancy

		(1)
Cohort group exposed to blackout:		weekly births
A.	exposure in third trimester	2.691
		(9.332)
B.	exposure in second trimester	-1.385
		(10.768)
C.	exposure before or within 6 weeks from conception	19.341*
		(11.555)
D.	cohort not exposed	-5.872
	(deliver: March 25-May 31)	(10.995)
	Observations	107

Robust standard errors in parentheses.

Regressions include quadratic time trends and month and year f.e.

*** p<0.01, ** p<0.05, * p<0.1

Table 11: Number of births and birth weights by mother's characteristic
Early exposure cohort

Type of regression	Number of births regressions			Average birthweight regressions	
	Weekly Average[a]	Coefficient on early exposure[b]	Magnitude[c]	Coefficient on early exposure[b]	Early exposure× characteristic
Baseline	173	19.34*	0.112	-77.1***	
		(10.83)		(21.9)	
Characteristic:					
Number of prior pregnancies:					
0	75.68	15.661*	0.206	-79.5***	10.4
		(8.420)		(25.9)	(26.2)
1	24.00	4.859	0.202	-78.6***	8.6
		(3.986)		(22.6)	(35.8)
2	17.77	5.706*	0.320	-75.7***	-15.2
		(3.070)		(22.3)	(44.1)
3	13.54	0.390	0.029	-75.6***	-15.2
		(2.590)		(22.1)	(57.4)
4	11.34	-3.759*	0.331	-72.4***	-48.0
		(2.197)		(22.1)	(58.8)
5	8.80	-0.754	0.085	-78.9***	47.2
		(1.862)		(22.0)	(66.0)
≥ 6	15.74	1.303	0.87	-78.2***	8.0
		(2.771)		(22.2)	(48.8)
Age:					
$\leq$15	1.63	0.970	0.595	-73.2***	11.6
		(0.662)		(21.8)	(190.2)
16-20	32.15	5.004	0.156	-70.2***	-16.5
		(4.581)		(23.1)	(29.4)
21-25	46.8	8.059	0.170	-74.8***	3.5
		(6.216)		(23.6)	(28.1)
26-30	43.65	4.611	0.105	-62.7***	-37.9
		(5.188)		(23.1)	(30.3)
31-35	23.14	2.730	0.117	-77.9***	29.7
		(3.417)		(22.2)	(45.0)
36-40	15.90	2.170	0.136	-77.5***	48.0
		(2.702)		(22.2)	(46.8)
> 40	3.60	-0.138	0.038	-73.8***	18.1
		(1.100)		(21.8)	(128.6)
Observations		119		18274	

First column: regressions based on weekly observations of the number of births. Each row is a regression, where number of births is determined by he maternal characteristic indicated in row title. Births from women with other characteristcs are not counted. Controls include year and month fixed effects and quadratic time trends. Second column: regressions are based on specification (3), where the set of controls is the same as in table 2 but excludes gravida for those regressions based on number of prior pregnancies, and age for those based on age.

a. Average number of births per week from mothers with specified characteristic

.b. Coefficient estimate of effect dummy (dummy takes the value of 1 during the period indicated).

c. Magnitude estimated as the fraction of effect over the average.

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Childbirth weight
effects of world prices of maize

	(1)	(2)	(3)	(4)	(5)	(6)
Early exposure	-67.8**	-78.7***	-70.1**	-78.5***	-75.6***	-62.6*
	(28.8)	(30.2)	(32.0)	(26.4)	(22.1)	(32.4)
Month 5	-53.6	-64.3	-58.3	-64.8	-54.4	-60.9
	(43.4)	(43.1)	(41.5)	(44.5)	(39.7)	(51.3)
Prices averaged over:						
Gestation period	-38.3					-518.0
	(81.6)					(632.5)
Year to birth		9.6				533.3
		(125.8)				(367.0)
1st trimester			-16.0			-79.1
			(56.4)			(243.8)
2nd trimester				5.2		197.0
				(57.6)		(211.7)
3rd trimester					-42.1	-45.7
					(66.0)	(238.6)
Observations	18195	18195	18195	18195	18195	18195
R^2	0.071	0.071	0.071	0.071	0.071	0.071

Regressions on child birth weight same as in table 7. Prices from FAO.
Prices for one tonne US Gulf yellow maize in 100s of dollars.
Heteroskedasticity-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Effects of employment structure on birthweight
Birth sample restricted to matched Labor Force Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES		No earnings			Including gross earnings		Including month 5 interactions	
Early exposure	-32.5	90.9	70.8	88.6	67.3	61.1	69.7	73.4
	(40.8)	(68.2)	(69.8)	(67.6)	(73.7)	(74.8)	(73.7)	(73.7)
Month 5	-64.8*	-64.6*	-91.2**	-64.9*	-66.1*	-70.5**	-40.6	3.2
	(34.8)	(34.6)	(34.7)	(34.6)	(34.7)	(34.6)	(47.1)	(78.7)
Gross earnings				23.2**	17.6		17.6	17.6
				(11.6)	(13.3)		(13.4)	(13.4)
Early exposure :								
$\times$ *ele_work*	-79.0	-889.0**	-900.2**	-891.7**	-1,153.8***	-1,155.6***	-1161.5***	-1,186.1***
	(110.3)	(370.3)	(373.1)	(368.2)	(361.0)	(363.7)	(360.6)	(360.6)
$\times$ ele_worksq^2		5,217.5**	5,343.4**	5,206.0**	6,299.1***	6,376.8***	6310.4***	6,467.5***
		(2,177.9)	(2,202.1)	(2,175.2)	(2,144.5)	(2,154.4)	(2,144.3)	(2,144.3)
$\times$ *earnings*					46.2*	47.6*	46.2*	46.2*
					(25.1)	(25.3)	(25.2)	(25.2)
Month 5 :								
$\times$ *ele_work*							-71.8	-365.3
							(108.5)	(413.3)
$\times$ ele_worksq^2								1,905.9
								(2,398.9)
Observations	11973	11973	11973	11973	11973	11973	11973	11973
R^2	0.072	0.073	0.074	0.073	0.073	0.073	0.073	0.073
Shehia f.e.	No	No	Yes	No	No	Yes	No	No

All regressions include mother's age, gravida, child's gender, twin and first pregnancy dummies, average education and wealth of household heads in the ward, month of exposure to blackout dummies, quarter and year of birth fixed effects. Robust standard errors in parentheses .*** p<0.01, ** p<0.05, * p<0.1

Table 14: Effects of domestic electricity on birthweight
Birth sample restricted to matched Labor Force Survey

	(1)	(2)	(3)	(4)
Birth weight in grams				
Early exposure	99.8	95.5	99.6	99.3
	(68.7)	(72.1)	(72.0)	(72.1)
Month 5	-66.1*	-66.2*	-22.1	-24.5
	(34.2)	(34.5)	(75.8)	(77.2)
Early exposure:				
× domestic electricity	93.5	178.5	164.6	172.4
	(103.8)	(255.6)	(256.8)	(264.3)
× domestic electricity squared		-78.2	-80.9	-88.1
		(220.9)	(220.2)	(226.2)
Month 5:				
× domestic electricity			-206.9**	-114.2
			(82.9)	(226.7)
× domestic electricity squared				-83.4
				(194.5)
Observations	11973	11973	11973	11973
R^2	0.073	0.073	0.074	0.074

All include early exposure interacted with work electricity and work electricity squared.
Additional controls are: mother's age, gravida, child's gender, twin and first pregnancy dummies, average education and wealth of household heads in the ward, month of exposure to blackout dummies, quarter and year of birth fixed effects.
Errors clustered at the ward level.
*** p<0.01, ** p<0.05, * p<0.1